Reply to Anonymous Referee #2:

Dear Anonymous Referee #2,

we thank you very much for taking the time to review our manuscript and for providing these detailed comments! We are especially grateful that you share your expertise on flooding/slushing/wetting of the snowpack with us during this review. Thank you very much! In the following, we will reply to all your comments sequentially.

General comments:

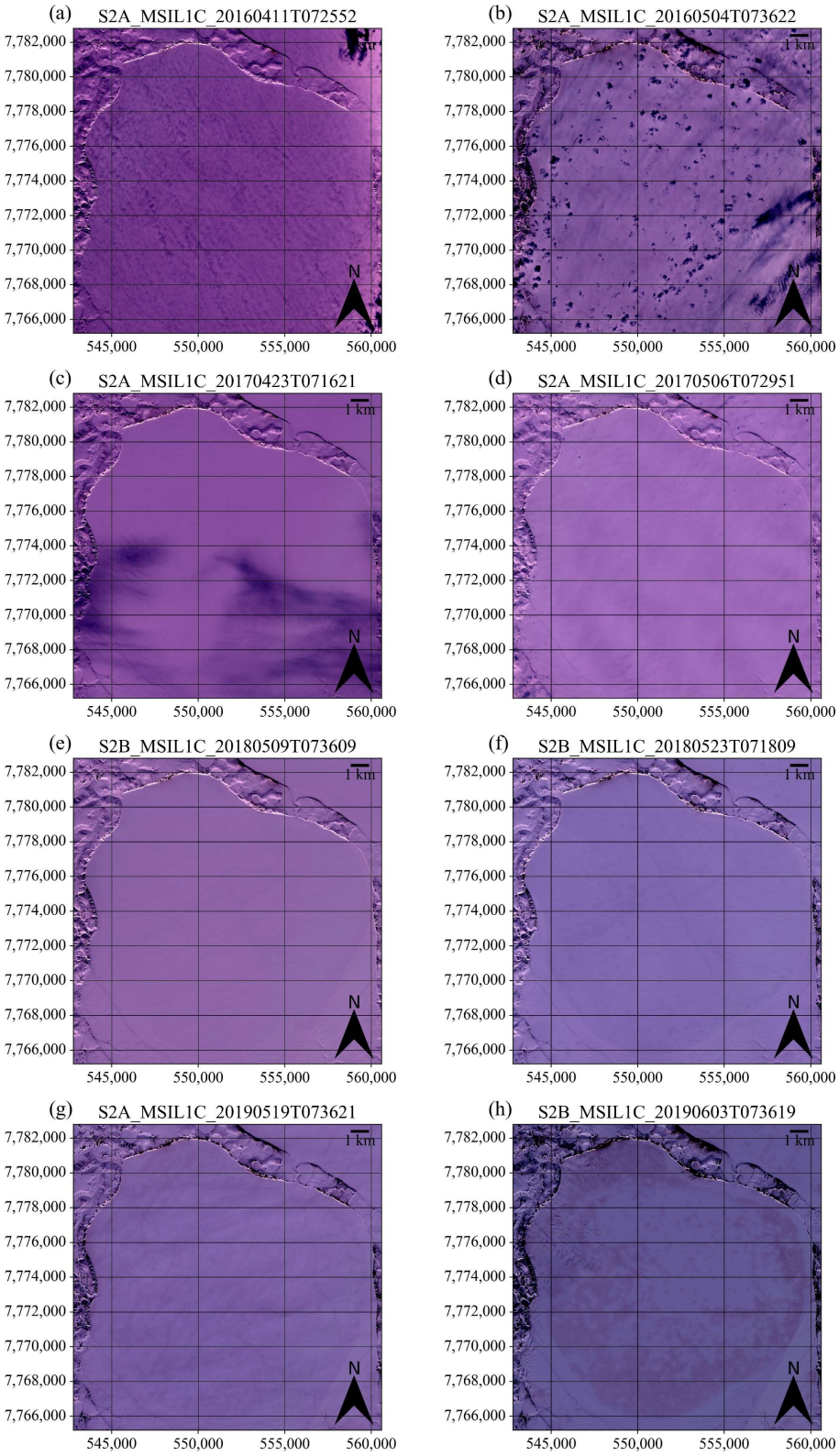
The manuscript presented is a detailed study of a single lake (Lake Neyto) in the Yamal Peninsula, which if the hypotheses are correct, present a method that could be used to monitor multiple lakes across a much larger area of both the Yamal and likely Siberian region. The methods presented in the study are technically sound, but the results are presented and interpreted to fit the narrative and at times, are cavalier by placing too much weight on hypotheses that do not have in-situ observations to back them up. The narrative of the paper hinges on the fact that methane ebullition is creating hotspots that are tens to hundreds of metres in area. The hot spots are spatially co-located with areas of open water that are observed in high resolution spring time optical acquisitions (WorldView 2), and when overlaid on SAR imagery, are also collocated with larger regions of lower backscatter.

Reply: We are pleased to hear that you think the methods presented are sound, thank you! We regret to hear but agree with you that the presentation and interpretation of the results is not adequate and should be revised. Please see the replies to your comments in the following!

The authors propose that the regions of lower backscatter are the result of methane ebullition that is creating large cavities in ice thickness, creating a specular reflection away from the sensor. This is difficult to agree with for a few reasons: First, as evidence in the Discussion section where the authors present evidence of surface slushing as a result of a hole being augered into the ice, the water level went approximately 40cm over the ice surface. This is significant, because if methane ebullition is creating holes or areas of thin ice, then the surface will undoubtedly become wet as the ice is depressed passed the hydrostatic water level. The slushy snow will either absorb incoming microwave radiation, or it will refreeze as snow ice (and become a greater backscatter). Since most of the lower backscatter areas increase throughout the winter season, it is more likely that the surface is becoming wetter as the ice is depressed by the increasing weight of the snowpack and water floods the ice surface. This is consistent with Figures 7, 8, and 9, as the area impacted by the hole is nearly always a concentric circle, consistent with water spreading on a (relatively) flat surface.

Reply: Based on your comments and further internal discussion, we think that flooding of the surface and consequent slushing/wetting of the snow is the most probable explanation for the observed patterns in the imagery. At first, it seemed puzzling that wet and/or slushy snow areas could expand so gradually over weeks to months. But given that with time the ice will get further depressed below hydrostatic water level with increased loading of (wet) snow and slush, this makes a lot of sense. Our expectation was that if flooding was responsible for the observed anomalies, we would be able to see indicators for flooding of the ice layer and/or slushing/wetting of the snow in most of the cloud-free medium resolution optical imagery acquired during late winter and spring (Sentinel-2 and Landsat).

Below is a figure with cloud-free Sentinel-2 images of different years (TOA reflectance, scaling for visualization between 0.7 and 1 to enhance contrast). The acquisition date and time is indicated in title (UTC). Local time is 5h later, so these images were acquired around 12:30 local time. Only in the latest acquisitions before or during melt onset we can clearly see similar patterns as in the SAR images.



The description in the manuscript was based on the expectation that there is significantly different reflectance in the flooded or slushed regions also in the earlier April images. This expectation might be therefore not adequate in this context.

Actually, a photo was taken (please see below) when the wheel of the all-terrain vehicle fell into the patch of very thin ice on of the lakes on Yamal (as stated in the preprint), which shows that there was fresh snow above the seep location. Before it was hit by the wheel, the site was indistinguishable from the surrounding snow-covered ice. The vertical structure before it was hit by the wheel cannot be

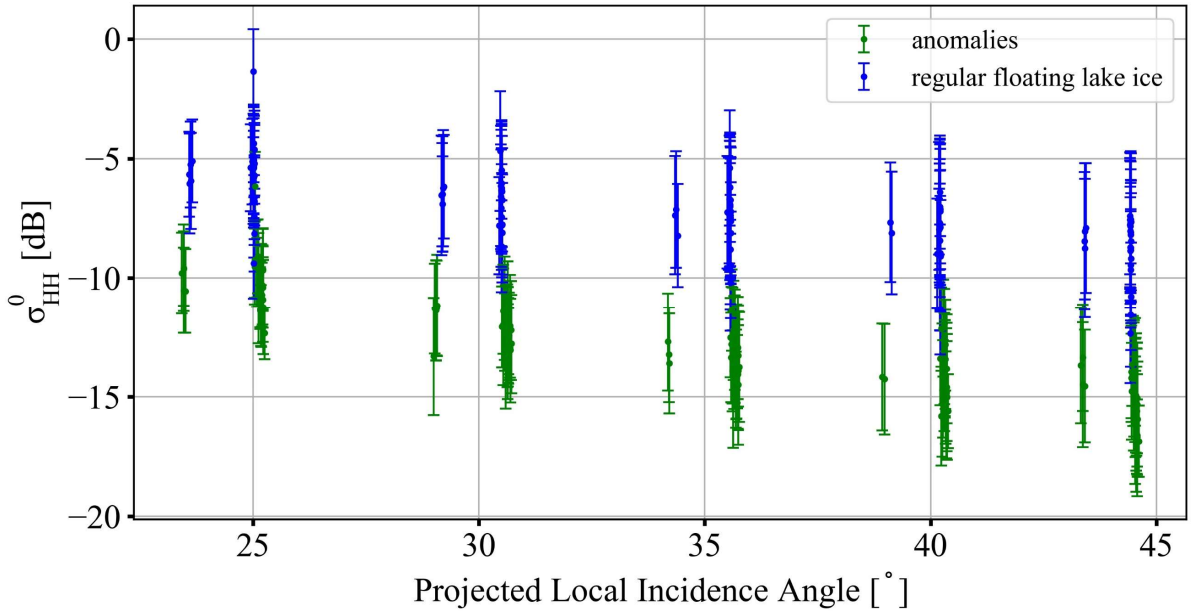
described with confidence. There was certainly fresh show on top, below that might have been a layer of melted or frozen slush, but this cannot be stated with confidence. The ice below was very thin. These observations might be related to what we see in the Sentinel-2 imagery, but further observations are needed to understand this in detail.

We again thank you for encouraging us to think more about slushing/wetting of the snow. We agree that a potential revised manuscript needs to bring a lessening of the claims/assumptions about the mechanisms involved.

Second, if cavities that are present in the ice are large enough to act as a spectral reflector as opposed to roughness, then based on scattering theory the radar cross section from the target would be consistent regardless of incidence angle. The authors have normalized the incidence angles in this study, and it would be interesting to see if the NRCS is consistent across the incidence angle range observed.

Reply: For a prefect specular reflector, the NRCS would be consistent across the incidence angle range, but then we would expect the magnitude of the backscattered signal to be below the noise floor of the sensor. What we proposed was that the main mechanism could be specular reflection, but the backscattered signal could still include contributions from the rough ice-water interface in smaller regions of regular floating lake ice inside the resolution cell. We do not think that we can differentiate between cavities and wet snow based on the incidence angle dependence alone in this case. This has become less important now, as we agree with you that wet snow or slush is most likely responsible for the observed low backscatter. We have nevertheless plotted the incidence angle dependence for all acquisitions for which anomalies have been identified for both classes. The points indicate the median sigma nought, the whiskers the standard deviation. Blue is the regular floating lake ice class; green is the anomaly class. There seems to be a similar dependence for both classes. The backscatter-incidence angle relationship would be better assessable by taking samples from a single scene (assuming similar

environmental conditions over the entire scene) over the whole incidence angle range, but this was not possible here (because we could not sample anomalies over an entire scene extent).



Third, there are no in-situ observations. The authors rightly mention that this area is extremely difficult to get to, and that direct personal observation of the holes are not safe due to thin ice. This acknowledgement of the limitation needs to also bring with it a lessening of the claims/assumptions that the source of the hotspots is definitely methane ebullition, and the mechanism that influence the SAR scenes.

Reply: We agree. We see that parts of the discussion and conclusions sections need to be rewritten. We hope that you understand that we cannot present fully rewritten paragraphs or sections at this point in this reply.

That being said, there is considerable scientific merit to this paper in the methods, statistical tests, and results that it shows. In my opinion I believe that the paper will become acceptable after significant revision to ensure that interpretation of imagery lacking in-situ data remains inquisitive as opposed to prescriptive.

Reply: We are glad to receive positive feedback for the methodological parts of the paper, thank you! We agree that the interpretation should become: "inquisitive as opposed to prescriptive", thank you!

Specific comments:

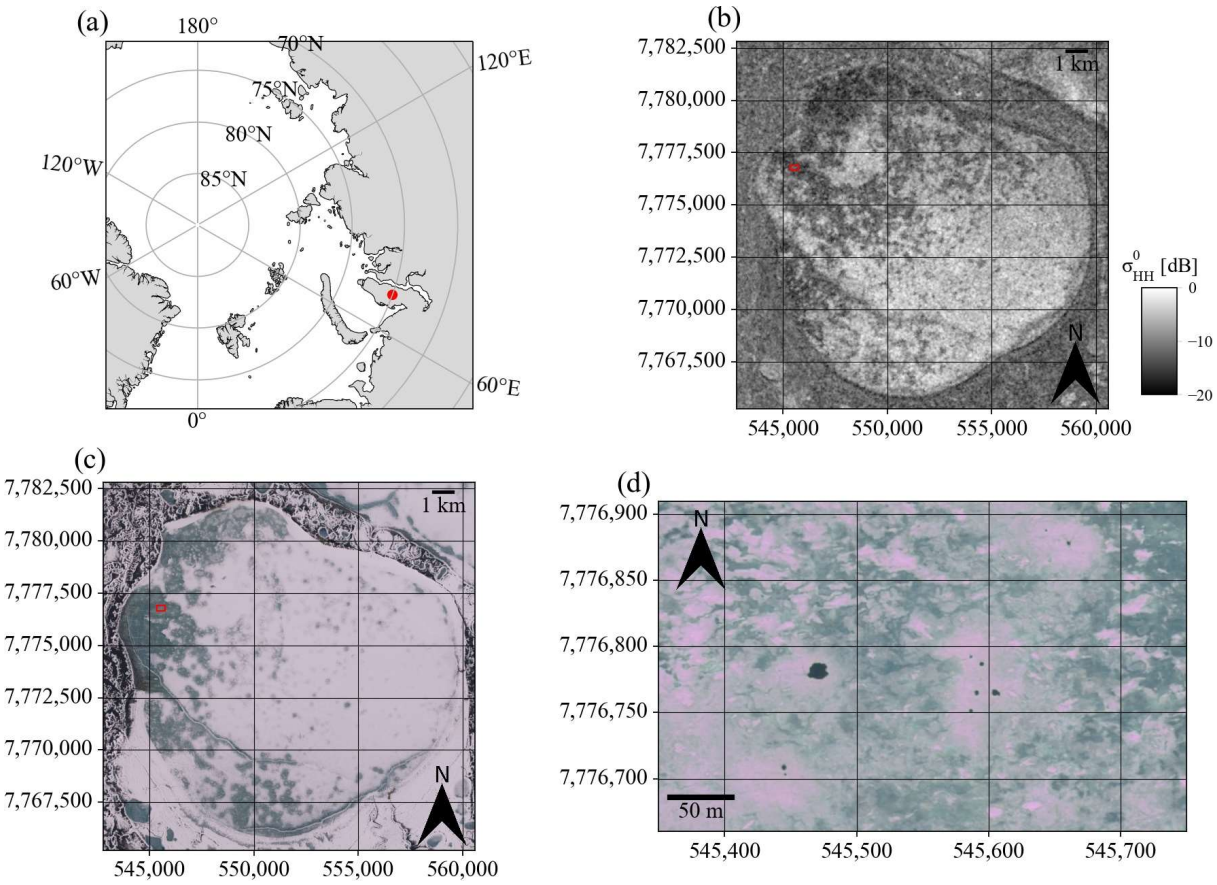
I will list line #s in this section, but firstly – this manuscript did not have a Study Site section. This is integral to this paper as it consistently references the surround region, and studies that have been done on other lakes. Please include.

Reply: This was also criticized by the other referee. We suggest adding a study site section, transfer lines 100-118 into that section (which would also reduce the length of the introduction) and adding the following additional information before lines 100-118:

"Lake Neyto (other title: Neyto-Malto), 70.073 °N, 70.350 °E, is located in the central part of the Yamal Peninsula, ca. 80 km away from the closest settlement Seyakha and ca. 80 km away from the Bovanenkovo gas field. The lake has the second biggest area (214 km^2) in Yamal after Yaroto-1 lake.

The length of the shoreline is about 60 km and the lake measures approximately 17.8 km in the south – north direction and 16.5 km from west to east. The lake is relatively shallow, reaching 17 m at the north-west corner, but the average depth does not exceed 3 m, which results in a significant mixing of water masses during summer (Edelstein et al., 2017). Wide shelf areas up to 800 m can be found within the lake, whereas at the deepest part, several depressions with diameters up to 500-800 m are documented (Edelstein et al., 2017). Lake shores are mostly cliffs up to 25 m high, sometimes with tabular ground ice exposures. The ground temperature at 2 m depth in the surroundings of the lake is approximately -1.5 °C (Obu et al., 2020). The Snow Depth Liquid Water Equivalent (SDLWE) generally increases gradually in winter and spring until melt-onset and typically ranged between 15 cm and 20 cm at its maximum in recent years (Hersbach et al., 2018)."

Additionally, we propose to include a fourth frame to Figure 1 indicating the location of lake Neyto following a suggestion by anonymous referee #1:



The Introduction section is very detailed but extremely long. Paragraphs between lines 55 and 95 can be further summarized to provide key points to the reader.

Reply: Agreed. These paragraphs should be further summarized to only contain the key points.

Page 1, Line 20: "Methane (CH4) is a powerful greenhouse gas and the global trend of its atmospheric concentration has shown significant changes over the last decades (Nisbet et al., 2014)." What changes" The concentration of Methane, or its effects? Please be specific.

Reply: The concentration was meant. We suggest adding the following sentence:

"Methane (CH4) is a powerful greenhouse gas and the global trend of its atmospheric concentration has shown significant changes over the last decades. The concentration increased significantly until

1998 and since 2007 until today, while between 1999 and 2006, it remained nearly constant (Nisbet et al., 2014)."

Page 2, Line 38: "150 thousand"

Please write as 150 000

Reply: We agree.

Page 2 Line 48: "… that gained a lot of attention in the scientific community recently."

What sort of attention? Newspaper? Scientific studies? Please provide references, and if they were the references earlier in the sentence, please provide at the end.

Reply: Basically both, newspaper and scientific studies. We now think this phrase sounds a bit odd. We suggest changing it to: "…such as a number of gas emission craters (GECs) that were discovered and described in recent years (e.g. Bogoyavlensky et al., 2016; Dvornikov et al., 2019; Kizyakov et al., 2020, 2017; Leibman et al., 2014)."

Page 3 Line 62: "Low radar return is observed from ground-fast lake ice due to low dielectric contrast between ice and the lake sediments (Duguay et al., 2002). On the other hand, strong reflection of the radar signal occurs at the ice-water interface of floating lake ice because of high dielectric contrast between ice and liquid water (Duguay et al., 2002; Engram et al., 2013)."

Provide the actual real and imaginary values of the relative permittivity so the reader can understand what a high and low dielectric contrast are.

Reply: We agree. These values are obviously dependent on the radar frequency and temperature and consequently need a more precise description. We suggest adding the following:

The dielectric contrast is determined by differences in the complex-valued relative permittivity ε, that in general depends on the radar frequency and temperature. The real part ε' of ice is approximately 3.17 and nearly independent of radar frequency and temperature (Mätzler and Wegmüller, 1987). The imaginary part ε" is below 10^{-3} for pure and impure freshwater ice at C- and L-band frequencies (Mätzler and Wegmüller, 1987). Meissner and Wentz (2004) provide a detailed list of the relative permittivity of water at various frequencies and temperatures. At 1.7 GHz and 25°C, ε' is 78 and ε" is 6. At 5.35 GHz and 25°C, ε' is 73 and ε" is 19. At 5 GHz and -4°C, ε' is 65 and ε" is 38. The relative permittivity of frozen soil largely depends on the temperature, water content, clay content, silt content and sand content (Zhang et al., 2003). ε of frozen soil was estimated from figures provided by Hoekstra and Delaney (1974) at 10 GHz and ε' is expected to range from 3.2 to 8, ε" is expected to range from 0.1 to 2. Little sensitivity of the relative permittivity of frozen soil to the radar frequency between 1.4 and 10.6 GHz is suggested by estimates by Zhang et al. (2003).

Page 3 Line 68: "Coming back to gas emissions",

Remove – this is unnecessary

Reply: Agreed.

Page 5 Lines 119 – 128: Understanding such phenomena can be important for numerous reasons, such as climate modelling, where global models 120 currently incorporate methane release from permafrost environments only poorly (Turetsky et al., 2020) and only consider ebullition from superficial seeps, or the understanding of sub-lake permafrost dynamics (Pointner et al., 2019). Another important point is that gas emissions can pose serious threads to humans, e.g. people working in the gas industry or local indigenous people. The Yamal-Nenets are reindeer herders that travel

across the Peninsula throughout each year. They frequently crossfrozen lakes in winter. In June 2017, a powerful explosion from a gasinflated mound that formed under a riverbed near Seyakha 125 on the Yamal Peninsula has been documented by Bogoyavlensky et al. (2019c), scattering debris over a radius of a few hundred metres. For lake Otkrytie, an eruption that seems to have been capable of breaking lake ice of 1.5 m thickness was described by Bogoyavlensky et al. (2019a). Understanding where different forms of gas release happen may be favorable for identifying areas of increased risk for humans."

This paragraph is out of place here. It should be moved to the beginning of the Intro or in the Discussion section to provide information about the impact of the study

Reply: We suggest moving it to the beginning of the introduction.

Page 6 Line 129: The Data section should have a table of the acquisitions that were used in this analysis for reproducibility. It's also important to list the relevant metadata about those acquisitions, specifically the local time of acquisition and the incidence angle. For example, you have several scenes that were acquired during days in which the temperature exceeded 0C. A daytime/nighttime acquisition time becomes quite crucial to your study then.

Reply: We suggest including this table as a supplement listing the scene ID, mean incidence angle over the lake (masked by the lake masks), UTC time and local time, since more than 300 scenes were already used for calculating the time series and this table would in our opinion be too large to be included in the manuscript directly. In particular, we propose to include 4 tables in total: One for all EW scenes used for calculating the time series, one for all IW scenes used, one for the EW scenes used for calculating the lake masks and one for the EW scenes used for calculating the shelf masks. What should also be included in the manuscript is that some products had to be assembled using the "slice assembly" operator in SNAP, when products have been sliced directly over the lake. We also suggest indicating these products in the tables and report a common mean incidence angle for them over the lake. If we were asked to resubmit the manuscript, we would suggest including the tables as supplement in ".csv"-format. We have already prepared these tables. Please see as an example the table for the scenes used for calculating the lake masks below:

scene ID	local time datatake start	mean projected loical incidence angle over lake	slice assembled	utc time datatake start
S1A_EW_GRDH_1SDH_20141005T020153_20141005T020253_002688_003004_6547	2014-10-05 08:01:53+06:00	35.6		2014-10-05 02:01:53+00:00
S1A_EW_GRDM_1SDH_20150922T124914_20150922T125018_007828_00AE74_FDE1	2015-09-22 17:49:14+05:00	43.4		2015-09-22 12:49:14+00:00
S1A_EW_GRDM_1SDH_20160913T122532_20160913T122632_013034_014A68_E6A2	2016-09-13 17:25:32+05:00	29.2		2016-09-13 12:25:32+00:00
S1A_EW_GRDM_1SDH_20170926T015430_20170926T015530_018540_01F3F2_4166	2017-09-26 06:54:30+05:00	40.2		2017-09-26 01:54:30+00:00
S1A_EW_GRDM_1SDH_20180926T020249_20180926T020349_023863_029AB4_C93D	2018-09-26 07:02:49+05:00	35.6		2018-09-26 02:02:49+00:00

The products listed above were not slice assembled. In case of slice assembly, this would be indicated by a "Yes" in the "slice assembled"-column.

Following a suggestion by reviewer 1, we propose adding a table showing the number of Sentinel-1 EW images used and their average temporal gap for each of the years directly in the manuscript:

Year	Number of images	Average temporal gap
2015	29	4d 7h
2016	88	1d 13h
2017	112	1d 7h
2018	52	2d 23h
2019	41	3d 14h

We suggest to also show a table for the other data used. By addressing your later comment, Figure 11 in the preprint (which showed Sentinel-2 images during lake ice break-up in 2019) is planned to be removed. With that, a single acquisition per satellite would remain. Please see the suggested table for the sensors other than Sentinel-1 below:

Platform	scene ID	local time datatake start	mean projected local incidence angle over lake	utc time datatake start
Sentinel-2A	S2A_MSIL2A_20160521T072952_N0202_R049_T42WWC_20160521T072949	2016-05-21 12:29:52+05:00	-	2016-05-21 07:29:52+00:00
WorldView-2	103001005502AD00	2016-05-22 13:03:13+05:00	-	2016-05-22 08:03:13+00:00
Landsat 8	LC08_L1TP_165011_20150406_20170410_01_T1	2015-04-06 12:03:51+05:00	-	2015-04-06 07:03:50+00:00
ALOS-2	ALOS2048741410-150418	2015-04-18 23:29:52+05:00	33.0	2015-04-18 18:29:52+00:00

Page 7 Line 154: 1236.5 MHz and 1278.5 MHz Use GHz or MHz – be consistent.

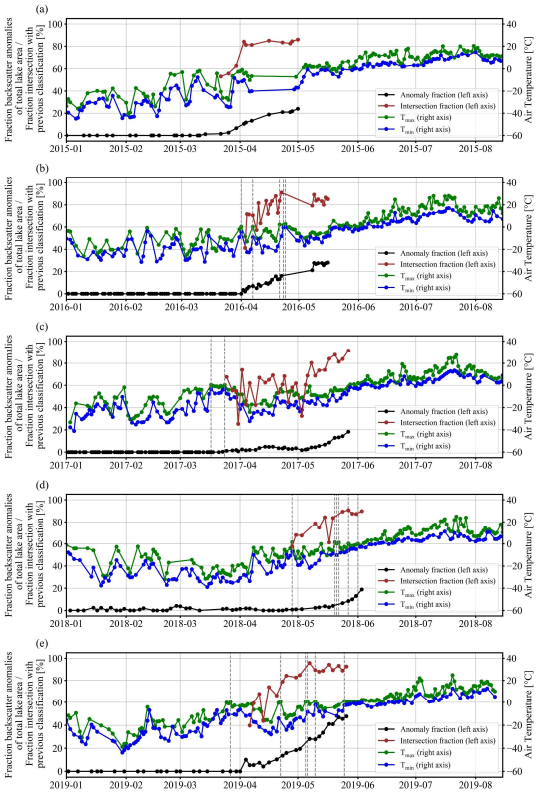
Reply: Agreed. We suggest using GHz.

Page 7 Line 180: "closest to lake Neyto and located on the east coast of the Yamal Peninsula at a distance of approximately 80 km, to assess potential temporal relationships between backscatter anomalies and air temperature"

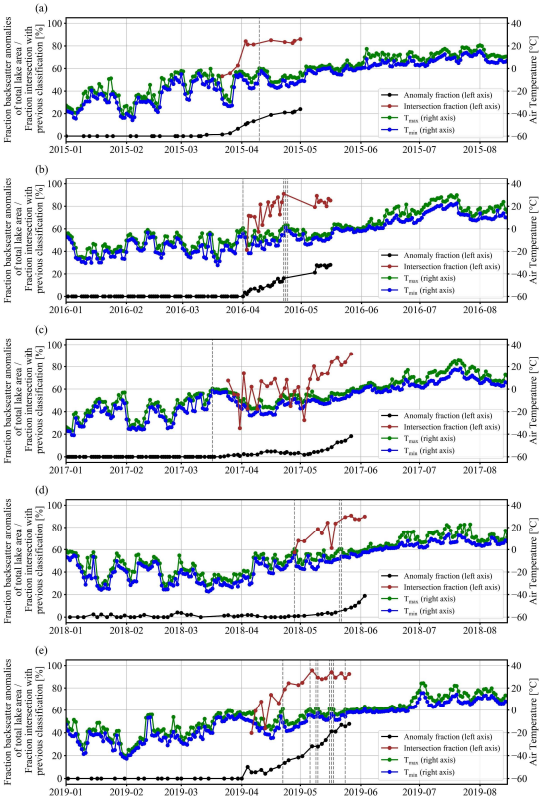
80km is a significant distance when considering air temperature, and the fact that the Seyakha station is located on the coast and lake Neyto is located in land of the Yamal Peninsula. Is it possible that a gridded reanalysis product would be better representative?

Reply: Yes, we have also mentioned in the discussion that this station is coastal and at a significant distance. We suggest using "ERA5 hourly data on single levels from 1979 to present 2m temperature" data (Hersbach et al., 2018) for the nearest gridpoint (70°N, 70.25°E) instead. We have already recalculated results using these data. Below is a comparison between the old (with Seyakha station data) and the new (with ERA5 data) plot.

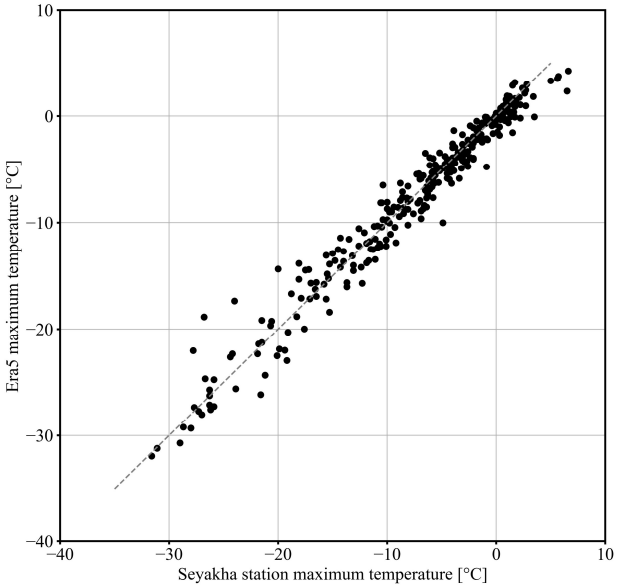
Old:



New:

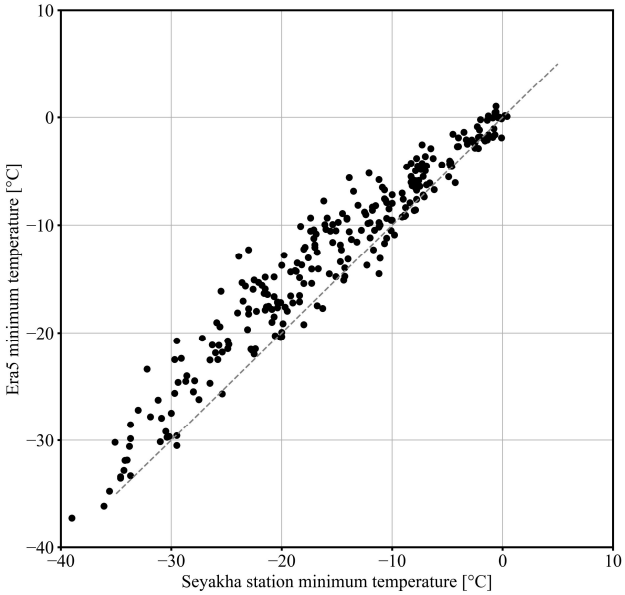


Please find below a scatterplot for the maximum temperature (between Seyakha station and ERA5 data) where station data was available for the most important months March to June in all years 2015-2019:



Temperatures look mostly similar, but differences around the freezing point can be up to a few °C.

When viewing the same plot for the minimum temperature data, a significant bias can be identified:



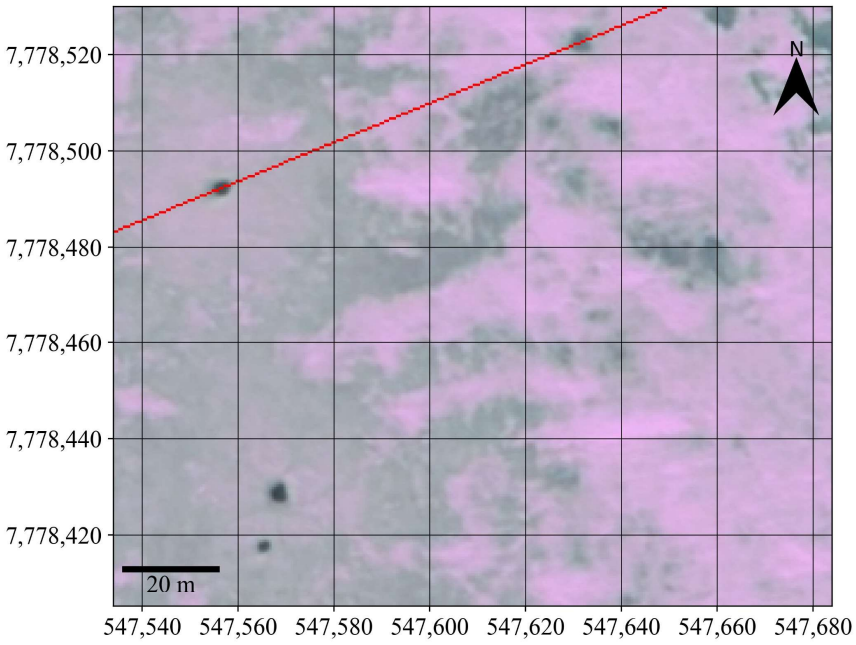
We therefore think it is indeed more appropriate to use the ERA5 data instead, as you suggested. Another advantage of using ERA5 data opposed to the Seyakha station data is that it does not have temporal gaps.

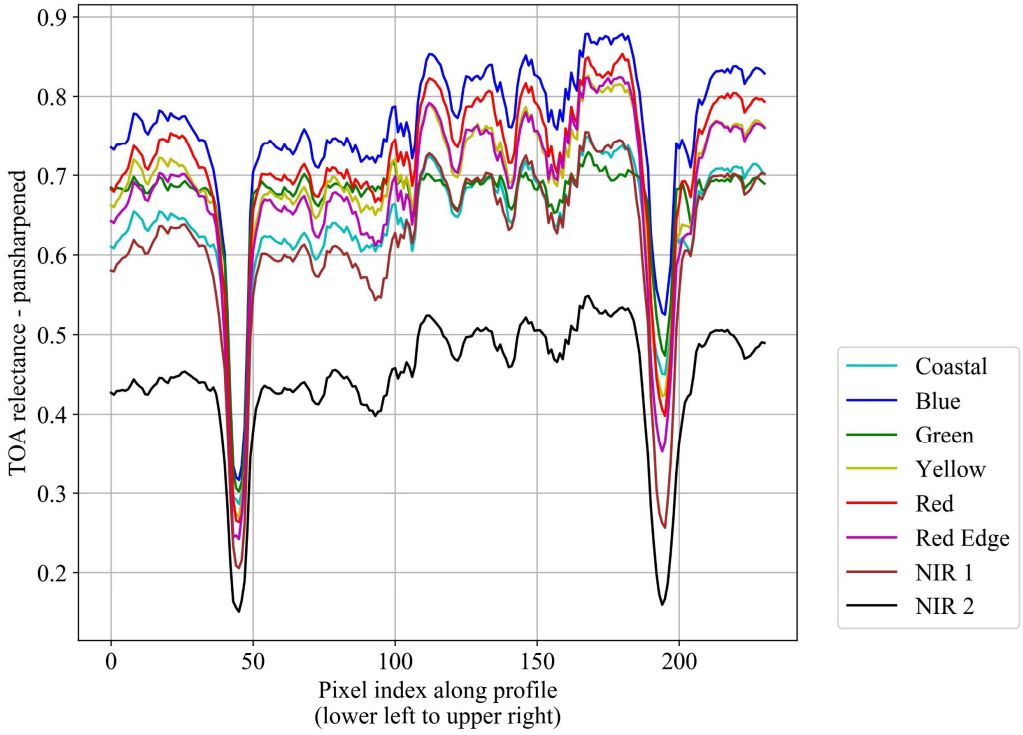
Page 8 Lines 183 – 187: "2.7 ArcticDEM digital elevation model V3.0 The ArcticDEM is a high-resolution, high quality, digital surface model (DSM) of the Arctic created by the Polar Geospatial Center (PGC) at the University of Minnesota from optical stereo imagery acquired by the WorldView-1, WorldView-2, 185 WorldView-3 and GeoEye-1 satellites using photogrammetric methods (Porter et al., 2018). Its spatial resolution of 2 m is unprecedented for digital elevation models (DEMs) with a pan-Arctic extent. The ArcticDEM was used for the terrain-correction of all SAR data presented in this study." This just doesn't need to be in here. The mention of ArcticDEM can be provided in Section 3.1.1., but is not necessary to the level of detail

Reply: We agree.

Page 10 Lines 265-266: "We used the green band as the input as it showed the highest contrast between the holes and areas of surrounding ice" This is surprising. Not the NIR band? It would be good to see a breakdown with a profile of reflectance, for instance.

Reply: We think we have not used the appropriate formulation here. Please see an example profile below:

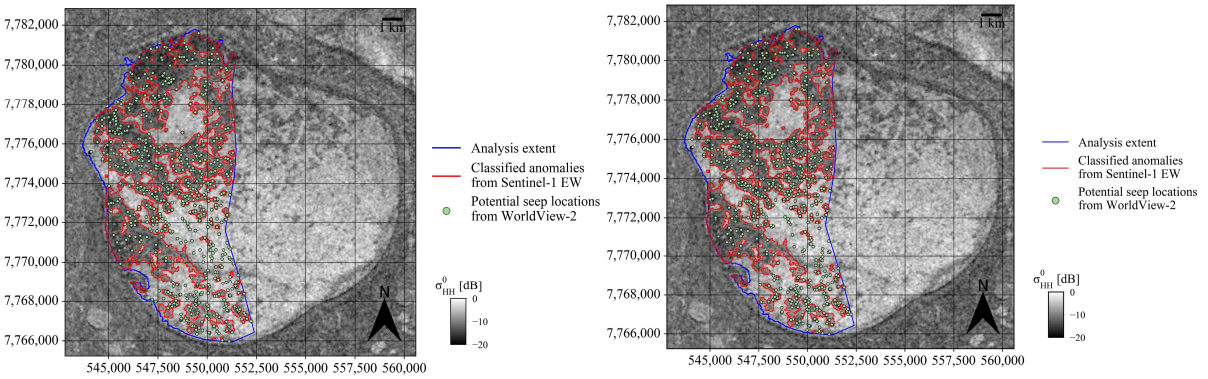




The left minimum is considered a hole that should be detected, while the right minimum is not. One can see that the contrast for the right minimum is the lowest in the green band. The green band allowed for the best separability. We suggest changing the sentence to "We used the green band as the input as it allowed for the best separation between holes and other surface features that we did not interpret as holes but could have been confused with holes by the blob detection algorithm". What was really required for this approach was the pan-sharpening. So, keep in mind that values indicated above are "pansharpened reflectance", that do not have a precise physical meaning anymore. We suggest to also add this figure to the manuscript for clarification.

In the preprint, we have used digital numbers (DN) and used the 5 bands whose wavelength range lies completely within the wavelength range of the panchromatic band for the pansharpening. We suggest using calibrated TOA reflectances with the use of all bands in the pan-sharpening (as this was required to produce the profile described above) if we were asked to submit a revised version. We have already recalculated the results using these new settings. Results are similar to the ones in the preprint. For example, please see the recalculated Fig. 7 below:

Old: *New:*



Please find comparisons of statistics between the old (with DN) and new (with TOA reflectances and pan-sharpened using all bands) approach below:

	Old	**New**
Number of detected holes	715	718
Number of hole polygons excluded for calculating histogram using area threshold	5	10
Median hole area	4.25 m²	4.0 m²
Percentage of holes inside classified anomaly regions	68%	71%
Mean minimum distance between the points (detected holes) and the polygons (anomaly regions)	48m	38m
Median distance of all points (detected holes) lying outside the polygons (anomaly regions)	97m	67m

Page 11: Table 1

This needs to be in the Data section

Reply: We agree. It should be moved to the data section. In addition, local acquisition times could be included if we were asked to submit a revised version.

Page 11 Lines 295-296: “We estimate the total number of pixels in the negative class (regular floating lake ice) to be about one order of magnitude larger than the total number of pixels in the positive class (anomalies) in the validation dataset (Table 1)” Where is this assumption coming from? Please provide.

Reply: This assumption comes from the confusion matrix used for calculating the validation metrics shown in table 2 of the preprint. We agree that this should be provided.

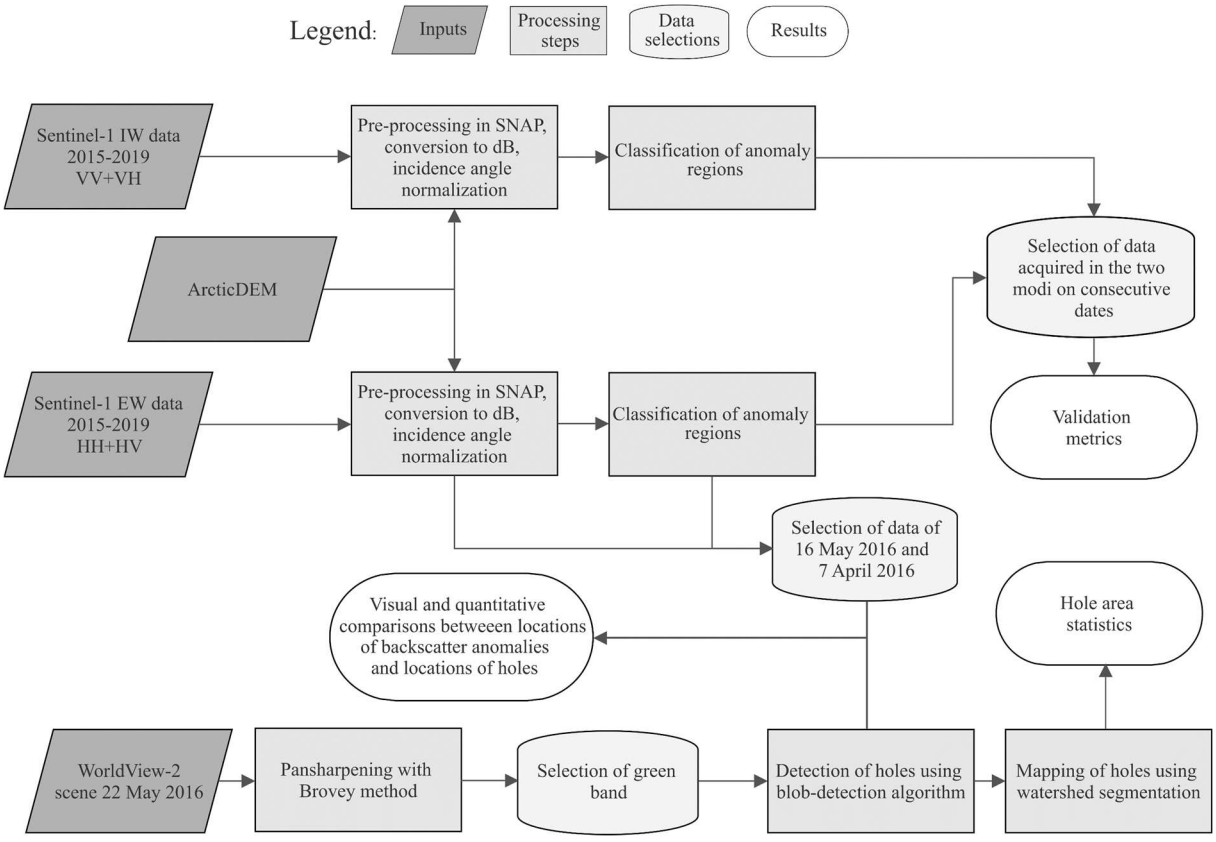
Page 12 Lines 311 – 318: In order to compare levels of σ 0 from anomalies when lake ice was present to those of open water on lake Neyto, we used all available Sentinel-1 EW and IW scenes acquired in July and August from 2015 to 2019, when the lake can be assumed to be largely ice-free. We masked the images using the same lake masks as described in Sect. 3.2.1 and calculated the mean σ 0 315 for the whole lake on single dates and averaged it over time, similarly to the calculations described in Sect. 3.3 above. We calculated the difference between this temporal mean of assumed open-water

backscatter and the temporal mean of the positive (anomaly) class backscatter (see last paragraph in Sect. 3.3). Again, all calculations were performed separately for each polarisation channel." This method has some pretty important flaws. As mentioned later in this article, open water backscatter is likely to be influenced by Bragg scatter due to waves, and slight waves on the order of 3cm can cause considerable bbackscatter of the signal. Holes in the ice would not exhibit this same kind of wave action. How can it be certain that we're comparing apples to apples here?

Reply: You are right about the influences. Since we now agree that scattering most probably comes from wet snow and/or slush, we think this comparison of backscatter levels is not useful anymore. We suggest removing it.

Figure 2: The workflow is not referenced anywhere in the paper. Also, it's confusing. The input data and actions are the same colour/shape, and the other symbols don't follow a similar structure. Please revise to be consistent. It also needs a legend to delineate input/output/method.

Reply: This section was intended to provide an overview of the most important methodological steps using the Sentinel-1 and WorldView-2 data. We agree that the figure should be better described in the text. We suggest changing the heading "3.5 Workflow visualisation" to "3.5 Summary of the most important methodological steps" and provide a short summary text on this section. This is the suggested new figure:



Page 19 Lines 345-346: "The majority of holes is characterised by an area smaller than 5 m2 , the median is 4.25 m2 . Few holes with areas larger than 100 m2 were identified."

How is it that we can detect holes that are smaller than 5 square metres? Also, that would mean that you're assuming that the cavities in the ice are much, much greater than 5 square metres based on the

area of low backscatter surrounding each hole. This does not seem practical compared to the likelihood that the surface snow is being wetted, and is absorbing the incoming microwave signal.

Reply: One pan-sharpened WorldView-2 pixel is 0.25 m² in area. What we replied above was that we (probably wrongly) had assumed that we would have been able to see effects of flooding more often on medium resolution optical images. Based on this assumption, the most probable explanation left was that the low backscatter had to do with the under-ice properties (cavities). We now agree that this does not seem practical.

Page 20 Line 354: "Figure 8 shows the same locations of detected holes deduced from the WorldView-2 image acquired on 22 May 2016 as in Fig. 7 on top of a Sentinel-1 EW HH-polarised acquisition from 7 April 2016, taken more 355 than a month earlier than the image in Fig. 7."

What was the temperature on 22 May 2016?

Reply: According to the ERA5 data, minimum temperature was -2°C and maximum temperature was 1.2°C. This could also be added in the text. Measurements at Seakha station were -1.4°C and 1.4°C for minimum and maximum temperature, respectively.

Page 21 Line 359: "A steady increase of area of backscatter anomalies in late winter and spring is evident. The maximum extent of backscatter 360 anomalies was especially high in 2019, where on the last useful acquisition date, its area was approximately half of the whole lake area (Fig. 9, compare also to Fig. 3 (a)). " Its evident that the intersection also increases when the air temperature is close to 0C or higher. This is very important, because slushy snow would be present during the same period, especially if they are located next to holes that are 40cm below the hydrostatic water level.

Reply: Yes, thank you for pointing this out. We would mention it in the text if we were asked to submit a revised version.

Page 21 Line 361 – 362: "The total lake area is approximately 200 km2 . Maximum air temperature is often approaching or slightly exceeding 0 ◦C throughout the analysis periods" Seyatha station is also coastal, which is in contrast to the region surrounding the lake. I'm not confident that a direct comparison is appropriate.

Reply: Yes, we intend to use ERA5 data instead and already recalculated results using these data, please see the reply to the other comment above.

Page 25 Lines 378-382: Potential signs of gas emissions might also be seen in Sentinel-2 optical acquisitions of the lake during melt and lake ice break-up. In 2019, a comparably high number of cloudfree Sentinel-2 acquisitions were taken during these time periods. 380 Figures 11 (a)-(e) show Sentinel-2 true-color composites for a section in the Northern part of the lake during melt and lake ice break-up in 2019. Irregularities in snow cover on top of the lake ice may be seen in Fig. 11 (a) and (b), while diminishing patterns of bright ice and dark spots not much larger than the pixel resolution are likely depicted in Fig. 11 (c)-(e)."

This is a leap, as the pattern in these images is very consistent with breakup of lakes with no methane ebullition.

Reply: Thank you for sharing your experience and for pointing this out! We suggest the removal of the figure and the accompanying text.

Page 27 Lines 394 – 396: "This result appears especially 395 significant when considering that the holes were mapped at 0.5 m pixel-spacing and anomaly regions from Sentinel-1 at 40 m pixel-spacing." Why could this be? Sentinel 1 acquisitions with a 40m pixel spacing could not resolve the holes, no. And it's

unlikely that the cavities will be over 200m in diameter. You have also presented that when augering into the ice that the ice is so depressed that the surface is wetted up to 40cm above the ice level. This evidence makes me invoke Occam's razor that the most likely result here is that the hole is influencing flooding of the ice surface and slushing events.

Reply: Based on what we stated above, we thought cavities might be the most likely explanation, but we agree now that flooding and slushing events are the most likely explanation.

Page 27 Lines 397 – 400: "As snow seems to have melted earlier in zones where anomalously low backscatter was observed before and the blob-detector algorithm was especially used to detect holes characterised by high contrast to surrounding bright ice, there could be more seeps that either do not form holes in the ice, are characterised by lower 400 contrast in zones with more snow, or both." This is less likely than ice pushed below the hydrostatic water level with a hold nearby.

Reply: We agree. Please also see the previous replies!

Page 27 Lines 404-406: "However, we are not aware of any 405 studies reporting such causes for shallow Arctic lakes and based on studies by Bogoyavlensky et al. (2019a, 2018, 2016) and Kazantsev et al. (in review), we consider gas emissions as the most likely explanation." This line is carrying a lot of weight, and needs to be validated.

Reply: You are right. We suggest changing it to "Ebullition of geologic methane as the cause of the holes in the ice would be consistent with studies by Bogoyavlensky et al. (2019a, 2018, 2016) and Kazantsev et al. (in review), but in-situ measurements are needed to confirm this hypothesis."

Page 27 Lines 411 – 414: "Continuous seeping with durations of at least weeks to months, associated with continuously expanding cavities might be an explanation. On the other hand, it seems surprising that the strongest expansions occur in spring, where the largest ice thicknesses can be assumed." See snow slushing example

Reply: Yes. We suggest removing this paragraph. Please see previous replies!

Page 28 Lines 418 – 421: "In case of cavity formation, it could be that the backscatter level of many pixels in the anomaly regions in the Sentinel1 EW imagery (40 m pixel-spacing) is caused by a combination of lower backscatter from cavity regions (due to increased 420 specular reflection from the gas/water-interface) and higher backscatter from zones of regular floating lake ice, as the resolution is comparably coarse."

This sentence is hyperbole – Can you support this with other references or studies? If not, I suggest its removal.

Reply: We agree to remove it.

Page 28 Lines 422 – 429: "In 2016 in late April and early May, very low backscatter from the entire lake surface was observed, which suggests wetting or melting of snow on top of the ice took place during that period and backscatter was mainly governed by interaction with the wet snow (Duguay and Pietroniro, 2005). Consequently, images acquired during that time were excluded from the analysis 425 (Fig. 9 (b)). One ALOS PALSAR-2 fully polarised scene in 2016 was available, which was unfortunately acquired during this period and was thus also not used for the analysis of scattering mechanisms. However, ALOS PALSAR-2 fully polarised data from 2015, one year earlier than the WorldView-2 scene was acquired, were available. The shape and locations of backscatter anomaly regions vary significantly between different years (Bogoyavlensky et al., 2018; Pointner and Bartsch, 2020) (compare also to Fig. 1, Fig. 3 and Fig. 10), but the characteristic expansion is similar in all years

analysed, as discussed above." I'm not sure what we as the reader get out of this paragraph because you're discussing data that you did not analyze.

*Reply: The first part was intended to explain why there is a significant gap in the time series in 2016. We visually analysed these data. Very low backscatter was observed from the whole lake, so these images were excluded from the classification. We agree that the following part should be removed: "*One ALOS PALSAR-2 fully polarised scene in 2016 was available, which was unfortunately acquired during this period and was thus also not used for the analysis of scattering mechanisms. However, ALOS PALSAR-2 fully polarised data from 2015, one year earlier than the WorldView-2 scene was acquired, were available.*"*

Page 28 Lines 439 – 440: "At L-band, backscatter from anomaly regions is higher than from regular floating lake ice (Fig. 10 (c)), which is the opposite effect as for C-band (Fig. 10 (a) and (b))." That is not what you presented in Figure 10 though, you presented the T11 parameter which is not "the backscatter"

Reply: You are right, of course. We suggest removing this sentence. If we look closely, what is evident is that the high values of T11 are mainly observed from the outlines of the anomaly regions, what might potentially relate to scattering mechanisms from slush and/or wet snow. In case we were asked to submit a revised version of the manuscript, we would suggest changing it to "While T11 values are similar between many centres of anomaly regions and regular floating lake ice, high values of T11 are observed from the outlines of anomaly regions, which might potentially relate to different scattering mechanisms for slush and wet snow, but in-situ data are required to assess this and understand scattering mechanisms at both, C-band and L-band frequencies in detail."

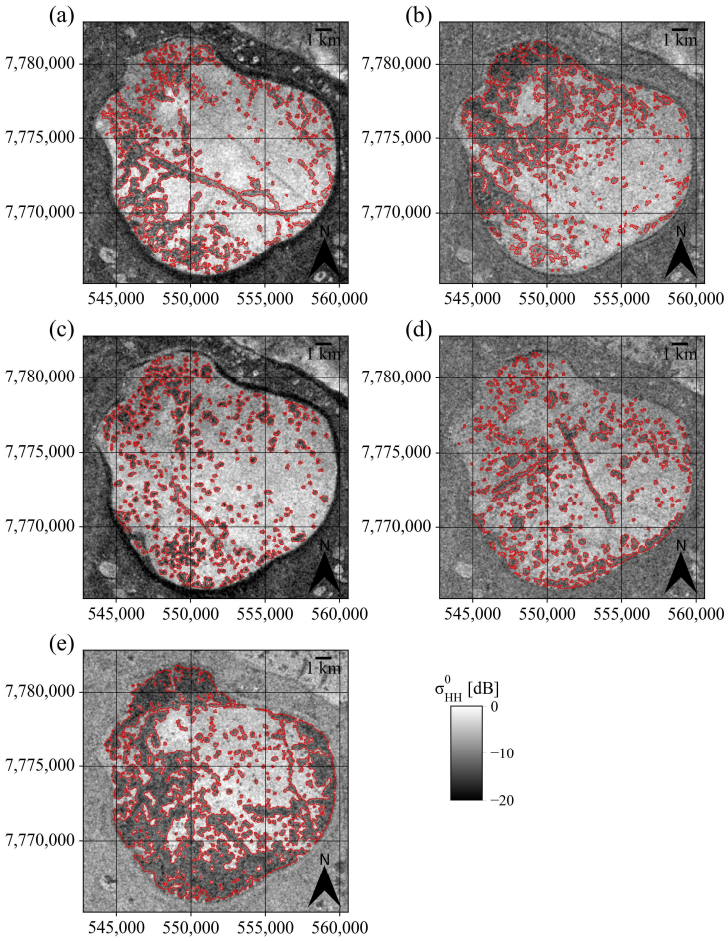
Page 28 Lines 450-451: "Another obvious difference between C-band and L-band is that backscatter from anomaly regions is higher at L-band (Fig. 10 (a), (b) and (c))." This was already stated above.

Reply: This was to compare the backscatter directly between C-band and L-band, while the previous statement compared backscatter between anomaly regions and regular floating lake ice for each frequency band separately. You rightly mentioned in the previous comment that it is of course T11 and not backscatter in general. We suggest removing this sentence and to use the formulation given in the previous reply.

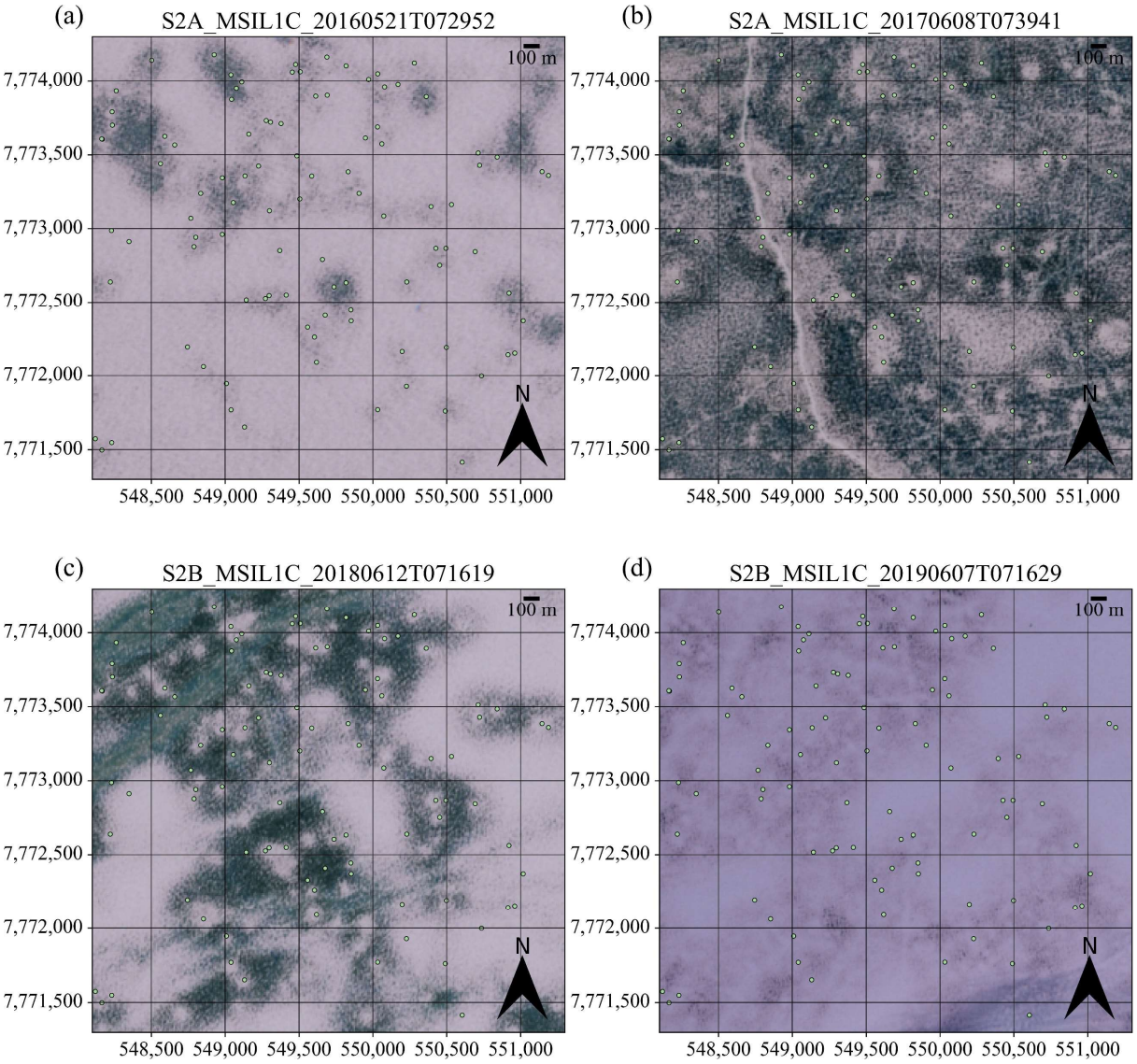
Page 29 Lines 458 – 462: "As a consequence of slowed ice growth, the cavities are filled by water, partly filled by gas or completely filled by gas (Engram et al., 2020). Resulting rough surfaces are the ice-water interface or the gas-water interface (Engram et al., 2020). For lake Neyto, formation of potential cavities (anomaly regions) could start in late winter or 460 spring and then the cavities may successively expand over time (compare to Fig. 9). Bogoyavlensky et al. (2018) and Pointner and Bartsch (2020) showed that locations of potential cavity zones (backscatter anomalies) vary significantly between years for lake Neyto."

It would make sense that the location of ebullition would remain consistent based on the source of ebullition. What biogeochemical process is there that you can justify the movement of the methane source? This needs to be addressed.

Reply: Indeed, this would make much more sense. Given that we assumed that it had to do with the under-ice properties, this was the only explanation for the large variations of locations of anomaly regions between the years. The following figure shows the anomaly regions in Sentinel-1 HH-polarized images of the last useful acquisition date in the years concerned, (a) 2015 to (e) 2019 (Pointner and Bartsch, 2020):



We now think that given what was discussed earlier, the changes in locations could be a result of different flooding/wetting/slushing patterns. So, the locations of ebullition sources could indeed most likely remain stable throughout all the analysed years. We have looked again on Sentinel-2 images acquired during melt-onset and there are (at least some) similarities between the identified points and patterns in the optical images in 2016 to 2019. The best explanation seems to be that the locations remain stable, but as you noted, of course it has yet to be verified that the holes are related to gas emissions.



Page 29 Lines 463 – 465: “Features related to ebullition responsible for increased L-band backscatter in PALSAR-1 SAR imagery in Engram et al. (2020) are of much smaller spatial scale than features that are expected to be responsible for 465 anomalies in SAR imagery of lake Neyto.” What are the features responsible in Engram et al., 2020?

Reply: The features are cavities at the ice-water interface that are (partially or completely) filled with water or gas. We suggest to explicitly state this in the manuscript.

Page 29 Lines 483-485: “Ice metamorphism processes related to increased solar radiation and air temperatures in spring such as the the formation of bubbles and air channels on the ice surface or the formation of ice needles 485 (Kouraev et al., 2015) may play a role, but this could not be assessed.” Slushing of the ice would happen during the winter season as well, not just the spring

Reply: Yes, we suggest removing this sentence.

Page 30 Lines 490-491: “During lake ice drilling on Yamal in April 2019, several lakes were found to have water level up to 40 cm higher than the level of lake ice. In situ observations of the lake ice of lake Neyto in winter or spring would be required to understand the cause of the anomalously low backscatter in detail.”

YES. This really provides evidence of what you're seeing in the SAR scenes. Based on the location of the holes and the area of low backscatter, the interaction has much less to do with the under-ice roughness/cavity, and much more to do with the absorption. Keep in mind that absorbed signals generally also show that they are the result of surface roughness in polarimetric decomposition (see target decomposition of first year sea ice, for instance). This sentence above supports the slushing hypothesis with in-situ observations of the snow/ice dynamics in the region.

Reply: Indeed. Thank you very much again for sharing your expertise! If we were asked to submit a revised version, we would use these in-situ observations as the support of what we see in the SAR images.

Page 30: Figure 12 In the caption, please provide the exact date of the observation, and the lake name (with coordinates)

Reply: The lake is termed "LK-013", observed and drilled on April 6, 2019 (ca 14:00 local time), 70.262123°N, 68.884803°E. Ice thickness at this place was approximately 1.5 m.

Page 31 Lines 503 – 510: "A steady increase of area of backscatter anomalies in late winter and spring can be seen in Fig. 9 for all years analysed. Especially high is the fraction of lake area covered by areas of anomalously low backscatter in 2019 (compare also to Fig. 3). 505 Also in 2019, a comparably high fraction of cloud-free Sentinel-2 observations were acquired during lake ice break-up. These acquisitions may show additional signs of degassing (Fig. 11, northern part of the lake). Regions that seem to have become snow-free earlier in Fig. 11 (a) and (b) partially match regions with increased frequency of dark spots in Fig. 11 (c), (d) and (e). Especially noticeable are diminishing patterns of apparently bright ice in Fig. 11 (c), (d) and (e). These bright patterns may show similar features as the WorldView-2 image acquired on 22 May 2016, but the limited spatial resolution of Sentinel-2 does 510 not allow to draw firm conclusions" Based on the discussion about this study, I believe that this paragraph is really too inconclusive to make any assumptions, and suggest its removal.

Reply: We agree to remove this paragraph.

Page 32 Line 539: "We do not claim that anomalies on these lakes are necessarily caused by gas emissions."

It appears that you have the same amount of evidence for these lakes as you do for Lake Neyto. It would be appropriate for you to state that the patterns are consistent with methane ebullition, but needs to be verified throughout the paper.

Reply: This was intended to show that similar anomalies also occur on other lakes to highlight the importance of studying these. We suggest stating that these patterns are consistent with what we observed for lake Neyto, but if these patterns are caused by methane ebullition needs to be assessed in future studies.

Page 33 Line 550: "anomalies are indeed likely caused by gas emissions through the lake sediments." Consider rewriting to read "anomalies are consistent with previous studies that quantify gas emissions…"

Reply: We accept this suggestion.

Page 551 – 553: ". The successive expansion of anomaly regions observable mainly during late winter and spring in all of the analysed years (2015 to 2019) might be explained by cavities formed by the gas emissions that successively hollow out the lake ice around seep locations over time." I disagree with this based on the evidence I have seen for the wetting of the snowpack due to overflow or through holes in the ice.

Reply: Yes, please see the previous replies. We suggest removing this paragraph.

Page 33 Line 560: “to the chosen validation strategy and could allow to monitor gas emissions on lake Neyto also in the future.” Consider adding “also in the future upon the verification of this hypothesis.

Reply: Yes, we accept this suggestion.

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